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VOL. XIV, No. 4. FEBRUARY 15, 1917

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

DOES HOLT FOLLOW FREUD?

HOLT'S recent volume, *The Freudian Wish and its Place in Ethics,* offers several points of interest. In the first place it shows that there is at least one psychologist in this country who, having had no first-hand acquaintance with Freud, yet has the courage to treat of Freud's place in a general system of psychology. In America there has been a Freudian cult—a shrine of worshipers made up of a few who have sat at Freud's or Jung's feet for some weeks or months. Returning to this country, these individuals have made it plain that the principles of Freud are not open to discussion by less favored mortals. Many of us have ventured to believe that even a mere psychologist can understand the essential nature of the Freudian mechanisms and that he can give them their place in the whole scheme of psychology. But we have been handicapped in our efforts to reach this goal by the lack of open discussion in our psychological journals. And the journals have been closed because no psychologist hitherto has had the courage—in the absence of hospital experience—to run the chance of non-profitable criticism at the hands of the favored few. This small band which has set itself up as the true exponent of Freud has done more to retard the broad understanding of that investigator than has the rather unfortunate way in which he himself has couched many of his illuminating principles. Holt's book will go far both towards breaking down the narrow and bigoted method of discussing Freud's theories and towards undermining the evil influence which his too particular adherents have exerted in this country.

Holt's praise of Freud is certainly unduly extravagant. After suggesting that Freud's contribution to science is epoch-making, he says: ". . . He has given to the science of mind a 'causal category'; or to put it less academically, he has given us a key to the explanation of mind. It is the first key which psychology has ever had which fitted, and moreover I believe it is the only one which psychology will ever need." The remaining part of the review will attempt to give Holt's justification of such praise.

Freud's key to the mind is called the "wish."[1] It is this wish which according to Holt transforms the principal doctrine of psychology and recasts the science, much as the atomic theory and later the ionic theory have reshaped earlier conceptions of chemistry. "This so-called 'wish' becomes the *unit of psychology, replacing the older unit commonly called 'sensation'* (italics mine); which latter, it is to be noted, was a *content* of consciousness unit, whereas the 'wish' is a dynamic affair" (p. 47).

Holt, in the chapter on the physiology of the wish tries—not wholly successfully—to come to closer quarters with the wish. He comes out with a conception of the wish which is wholly acceptable to a behavior psychology, but it is not Freud's wish. The wish Freud deals with is never defined, whether in psychological or biological terms. In certain places in Freud one gets the notion that he might be willing to see the wish expressed in terms of the reflex or in habit terminology, yet one must finally arrive at the conclusion that Freud never had any particular interest in working the wish out biologically or connecting it up with the well-known principles of habit formation. The wish remains for Freud a kind of mystical entity—a kind of disembodied ghost which arises to terrify the possessor, not once or twice, but continually. The non-biological or mystical nature of the wish has been even more clearly expressed in the writings of the colleagues of Freud. For example, in Jung's recent book on the unconscious the libido has become a kind of psychic energy—the amount of which in a given individual remains fairly constant, since if there is a lack of libido in one department of life, there is a too great supply (compensation) elsewhere. The wish becomes here little less than an *entelechy*—undisguised by even the thinnest biological raiment.

To bring the wish into agreement with a reflex or objective psychology has seemingly been Holt's chief undertaking, but yet he fails sadly in certain places in remaining objective. Disregarding some of the terminology which belongs to the realistic movement, he talks in general the language understood by the behaviorist. For him the wish includes whatever would be called an impulse, tendency, or attitude. "An exact definition of the 'wish' is that it is a *course of action* which some mechanism of the body is *set* to carry out, whether it actually does so or does not"[2] (pp. 3 ff.). In the chapter on the Physiology of Wishes he takes up the reflex arc con-

[1] Of Holt's facile and easygoing presentation of Freud's general theory I shall say little. The first chapter might serve as a good introduction to Freud and one safe enough to put in the hands of even elementary students.

[2] If the "wish" is the *key* to psychology and this is a definition of the wish I fail to see why the key has not been in the hands of psychologists since the time of William James.

cepts and lays the basis for his later discussion of the way "wishes" affect conduct. His criticism of the Meynert scheme and subsequent rewriting of it gives the closest approach to the central part of Holt's book. I quote him in some detail: "An interesting problem of a partially conflicting sensory pattern is 'the Meynert scheme' of the child and the candle-flame, which has become generally familiar owing to its having been quoted by James (*The Principles of Psychology*, Vol. I, pp. 24–7). Meynert aims to show by a diagram how a child learns not to put his finger into a candle-flame. Two original reflexes are assumed: one in which the visual image of the candle causes the child's finger to go out to touch the flame; the other in which the painful heat on the finger causes the child's arm to be withdrawn. A fanciful series of nerve-paths, fabricated in the interests of the 'association theory,' purports to show why after once burning himself the child will in future put out his hand, on seeing a candle, but draw it back again *before* he burns himself. The explanation is beautifully accomplished by begging the whole question; that is, by resting the 'explanation' on certain time (and strength) relations between the two reflexes of extension and retraction—relations which neither diagram nor text accounts for. In fact, apart from the passage in which the whole question is begged, both diagram and text show that on every subsequent occasion the child will infallibly put out his hand, burn it, and then withdraw it, just as he had done the first time; for the reflex path for extending the hand is the shorter and the better established of the two, and it remains entirely vague as to how the impulse to withdraw shall arrive in time to save the hand.

"But Meynert's explanation is not only unsuccessful, it is wrong in its intent. If achieved, it would show that a child once burned will on merely seeing a candle, and before it feels the candle's heat, draw back its hand. And this, Meynert thinks, is the process of learning. Whereas in fact a child that shrinks on merely *seeing* a candle has not *learned* anything; it has acquired a morbid fear. So far from being a step in learning, such a reaction will gravely impede the child in acquiring the use of this innocent utensil. It is true that one severe experience of being burned can establish the morbid cringing at the mere sight of fire, but every teacher knows how disastrous this is to a child's progress; and the mechanism of such a response will not be found in any such figment of the imagination as that which Meynert adduces. I know of nothing in this 'Meynert scheme' that tallies with fact, and, as James well says, it is 'a mere scheme' and 'anything but clear in detail.' Nothing but the authority of the association theory ever loaned it plausibility."

The understanding of Holt's amended scheme is really essential

to his system of psychology and to his later applications to the problems in ethics, and yet it is just here that I must complain of "reviewer's dullness" or else lack of clearness of expression on Holt's part. Holt's solution of Meynert's problem is stated as follows: "The normal process of learning to deal with a candle is the process of establishing a response to an object which is both luminous and hot, if we consider only the two properties so far brought in question. The successful response will be one which is controlled directly by the actual properties of the candle, for this alone means precision and nicety in handling it. The normal child learns the properties of objects, without acquiring a fear of these properties; for fear is *not* 'wholesome.' The case in hand is simple. The child has in fact the two original tendencies, to put out its hand to touch any pretty, bright object, and to draw back its hand when the nerves of pain are stimulated. But these are at first not coordinated; and coordination (learning) is the establishment of a just balance between the openness of the two paths; where 'just' means proportioned to the actual properties of the candle. On first seeing a candle the child puts out its hand; the second reaction (of withdrawal) is touched off by stimulation of the heat-pain nerves in the hand, and the moment at which this will happen depends on the sensitiveness of the heat-pain end-organs, and the openness of the path connecting them with the muscles that retract the arm; of which probably the openness of path is the modifiable factor. The warmth of the candle begins to stimulate this retraction reflex, and stimulates it more, and at an increasing rate of increase, as the hand approaches the candle. All this is needed to save the child from burning its hand, and this is what Meynert's scheme aims to explain, is an openness of the retraction reflex path sufficient to stop the hand before it actually reaches the flame. If the act of extension excited through the eye is not too impetuous, the retraction reflex will from the outset protect the hand; but if the former is a very open path, the advancing arm may get a momentum which the retraction reflex will not be sufficiently quick and strong to counterbalance in time to save the hand from being burned. A few repetitions of the experience will give this retraction an openness which will safeguard the hand for the future; and this process is aided by the prolonged pain yielded by a burn, which continues the retraction stimulus for a considerable period and so 'wears' down the retraction path more than a great many merely momentary stimuli could do. In this way a single experience of burning is often sufficient for all time. Thus experience establishes a balance between the two opposed reflexes, of extension and of pain avoidance, such that the organism carries on its further examination of the candle in safety."

Possibly these two quotations do not sufficiently present Holt's argument. I fail to see even in his more extensive presentation an improvement of Meynert's scheme. Meynert's scheme certainly begs the question, but so does Holt's. If Meynert's scheme demands neural association, so does Holt's. Holt tries to minimize the amount of neural association involved by saying a "few repetitions of the experience will give the retraction path an openness which will safeguard the hand for the future. . . ."[3] But this attempt to minimize the associational factors reminds one somewhat of the classical attempt to minimize moral delinquency by pointing to the small size of the baby. In these few experiences a genuine *learning process* is involved and the explanation of this learning process—regardless of whether the act is acquired in few or many trials—is what I consider one of the chief problems in psychology. Holt makes no problem of it at all. The problems which the reviewer and others have treated under the heading "fixation of arcs in habit" would thus seem to have small place in Holt's system.

In Holt's application of his opposed impulse theory to the field of ethics, he has been somewhat too abrupt. It is hard for the reviewer to see that there are many situations in the life of the child which are on a par with the candle. The baby has to form literally hundreds of thousands of such simple reactions to objects. As I look over them I fail to see how one per cent. of the situations in a baby's life square with the one described by Holt as the general one. I for one feel that Holt should have taken several less spectacular situations in the very early life of the child[4]—and some which offer a bit more resistance to his argument. Accepting Holt's argument at its face value, let us see how he makes his rather ingenious applications in the field of ethics. "An innate tendency or purpose of an infant is to put out its hand to touch fire. If the mother is by, she

[3] I wish here to enter a partial factual objection to Holt's arguments. In the case of two young children I observed, the candle training, given, to be sure, at widely separated intervals, required many separate tests, although the experiment was made complete in the sense that the candle was allowed to remain present until retraction occurred. I can see nothing in the baby's learning to react to a candle which differentiates that act from any other process of learning.

[4] In the later stages of development he has more or less made good this omission. "Thus, just as the leaves of certain plants, which are subject to the two impulses of facing the sunlight but also of avoiding desiccating heat, will spread themselves out broadly toward the sun in the morning and afternoon, but in the heat of noonday will partially fold up, so under the teacher's eye the pugnacious impulse of the small boy is subdued to the furtive expedient of the spit-ball; and so too, the man who yearns for worldly power but yet in *personal* contact with his fellows is unconquerably timid will become a renowned inventor, or a shrewd manipulator of stock-markets, or in politics will work into some important position '*behind* the throne.'" (See also pp. 115 ff.)

holds back the hand (*her* purpose) before it reaches the flame. There is a hint for the child, here, of right and wrong. If the mother guards the child unremittingly, and every time restrains the hand before the uncomfortable warmth begins to stimulate the child's own tendency to withdraw, the child will never be burned and may eventually (in a way to be described) acquire the habit of stopping short before reaching the flame. But this cautious conduct will not be guided by (be a function of) the heat of the flame, for the child has had no experience of this. The child's general conduct toward fire will then be partly a function of the immediate properties of fire (its color, position, shape, etc.); but partly also of a something else (really its mother), which may or may not figure explicitly in the child's field of consciousness. The mother has set a barrier between the child and a portion of reality; and forever after the child will be in some measure impeded in its dealings with fire. An inhibition of which the source of sanction is thus not intrinsic is precisely, I suppose, a tabu.

"Or again, if an equally unremitting mother lets the child put out its hand towards the flame and takes care only that the hand by too great momentum or an accidental lurch does not actually come into the flame, the child will not be burned and its own mechanism of withdrawal will be exercised not through the mother's interference but through the direct action of the flame's heat. The child's conduct toward fire becomes integrated, and is solely a function of the actual properties of fire. Ten years later you shall hear the first mother shouting, 'Bobbie, don't you dare put your hand so near the lamp, and if you touch those matches again your father will whip you.' And the second mother will be saying, 'Bobbie, go get the matches now and light the lamp, and set it down on the center-table.'"

I can not enter here into an analysis of his really brilliant chapter on the Wish in Ethics. Although I am not struck by the cogency of Holt's logic, I nevertheless feel impelled to say that this chapter is the first real contribution I have ever seen towards a functional ethics. Surely in future studies of ethics we can have less of Kant and the history of philosophy in general and a more genuine grappling with the real practical situations which lie at the heart of ethical conduct. I am tempted here to justify the title of this review by modifying a famous Whistler saying—"Holt has written an interesting chapter on ethics, but 'why lug in' Freud?" Ere this chapter is reached Freud's views have become submerged in Holt's.

That which interests me in Holt's book more than anything else is his behavioristic tendency. At one time I am ready to say "Holt is a genuine and consistent objectivist," as for example in his description (pp. 85 ff.) of the man purchasing the railway ticket:

"On seeing the man purchase a ticket at the railway station, we felt that there was more behind this action, 'thoughts' that were the invisible secret of his movements. Suppose, instead, we inquire whether the more is not ahead. More is to come; let us watch the man further. He enters the train, which carries him to a city. There he proceeds to an office, on the door of which we read 'Real Estate.' Several other men are in this office; a document is produced; our man takes a sum of money from his pocket and gives it to one of the other men, and this man with some of the others signs the document. This they give to our man, and with it a bunch of keys. All shake hands, and the man we are watching departs. He goes to a railway station and takes another train, which carries him to the town where we first saw him. He walks through several streets, stops before an empty house, takes out his bunch of keys, and makes his way into the house. Not long afterwards several vans drive up in front, and the men outside proceed to take household furniture off the vans and into the house. Our man inside indicates where each piece is to be placed. He later gives the men from the vans money.

"All this we get by observing what the man does, and without in any way appealing to the 'secret' thoughts of the man. If we wish to know more of what he is doing we have only to observe him more. Suppose, however, that we had appealed to his inner thoughts to discover the 'secret' of his movements, when we first saw him buying a ticket at the railway station. We approach him and say, 'Sir, I am a philosopher and extremely anxious to know what you are doing, and of course I can not learn that unless you will tell me what you are thinking.' 'Thinking?' he may reply, if he condones our guileless impertinence, 'Why, I am thinking that it is a plaguey hot day, and I wish I had made my morning bath five degrees colder, and drunk less of that hot-wash that my wife calls instant coffee.' 'Was that all?' 'Yes, that was all until I counted my change; and then I heard the train whistle.—Here it is. Good-by! And good luck to your philosophy!' "[5]

No behaviorist could give a better description of the situation than this. But yet in many other places in the book Holt finds it impossible to rid himself of subjective terminology. Indeed, his slipping over from one form of expression to the other is quite noticeable all through the book. An objectivist of my own type feels tempted in reading Holt's book to give him just a little push here and there to make him cross the line a little more squarely.[6]

[5] His objective tendency is again uppermost (pp. 125–30), where he ably describes in words wholly acceptable to the behaviorist the mechanism of suppression, dissociation, discrimination.

[6] Possibly it might be well to point out a few such places. He says (p. 16):

One may be disappointed in not finding anywhere in Holt's book a detailed statement of how he can substitute the unit concept "wish" for the introspectionist unit concept sensation. The reason for the omission is clear. When a unit changes in a science the problems and points of interest shift. Holt as a behaviorist has no interest in making the substitutions because his problems do not demand it. He is interested in conduct and, in the present book, in the ethical side of conduct. Nevertheless, I hope that at some future time Holt will do this and show how the conception of the wish as a unit differs in essential particulars from the reflex arc concept which the behaviorists had previously adopted. In a recent number of the *Scientific Monthly* I have tried in a popular way to rewrite the essentials of Freud's theory in terms of behavior. From the psychiatric standpoint it seems to me that the functional conceptions which Adolf Meyer has employed are nearer to Holt's point of view than Freud's very indeterminate concept of the wish.

One might get the impression, and with some justice, from Holt's book that exact experimentation along lines other than those marked off by Freud are at least quite secondary, but the mere fact that Holt has not given any closer contact with the mechanism of habit formation shows quite clearly that the scientific understanding of the *modus operandi* of even the disemboweled wish (*i. e.*, of the wish reduced to motor set or attitude) must await the working out of just those factors which the behaviorists insist upon.

I think it only fair to say that I have reviewed only those parts of Holt's book which particularly interest me. I can whole-heartedly say—before the author says it for me—that any one interested in the book as a unit or system must read the whole volume!

JOHN B. WATSON.

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"The 'still small voice' is the popular, but just designation for the protest of the semi-dormant upper self when, in revery, fancy, or imagination, lower impulses have succeeded in intruding on the field of consciousness." Again (p. 31), in describing one of Freud's books, one feels that Holt is almost "orthodox." "Of course he had unconsciously written 'calligraphy' instead of 'chirography'" (p. 32). "But habit and other preconscious motives exercise specific influence as well" (p. 33). On page 45 I again fail to grasp Holt's use of subconscious. He uses it three times on this page without quotation marks and apparently seriously. It is needless to go through the book in this way, but I can not resist the following quotations which come in after his description of the gentleman purchasing real estate: "Not that there are not actual thoughts, but tradition has turned thought into a myth by utterly misconceiving it and locating it in the wrong place." "Thought is often a mere irrelevance, a surface embroidery on action" (p. 87). Of course the book is only a sketch and it is unfair to criticize the author too severely, but he leaves me confused because in many places he uses thought, subconscious, etc., in quotation marks, while in others he apparently uses them as would the structuralist.

THE CONTRAST BETWEEN SCIENTIFIC THEORY AND THE DEMANDS OF THE PRAGMATIC PRESCRIPTION[1]

IN an earlier paper[2] it was pointed out that corresponding to any fact or law there is definable the set of conditions without which the fact could not be, or without which the law could not be even possibly true (much less probably true). It was shown that the opinion embodying these conditions is frequently known in science as a *theory*. The name, *theory*, was suggested for all such opinions. The theory is as true as the fact. The opinion answering to the conditions so defined would necessarily be workable in an extraordinary degree. Unless this opinion were the true one, things could not be observed to be as they are. The opinion then certainly is one which *works* for the observed facts; and it is this aspect of the matter—and to my mind it is the least important aspect, and certainly not the one which gives theory its claim to be true—that is universally emphasized in the textbooks of science.

The undulatory theory of light works better than the corpuscular hypothesis; it explains things which the discarded opinion could not explain. The kinetic theory of gases works so well that one can predict, with its help, laws which are actually verified. An important textbook of physical chemistry says:[3] "A theory, to be accepted as useful, must enable us to deduce known facts and new facts. The kinetic theory of gases certainly fulfils the requirement, for we can deduce from it the gas laws and Avogadro's hypothesis." The theory here, it seems, should be *accepted as useful*. It is wholly overlooked that the kinetic theory, in the main (in the main, for I do not think that every bit of the kinetic theory answers the requirement) is a statement of the conditions without which a large group of phenomena could not be. As such it is true. Now, I say, it is *not* remarkable that true premises should enable one to predict verifiable conclusions. For the true implies only the true. Logicians know, however, that the false also implies the true; and it may well happen that an hypothesis, though false, still leads to verifiable predictions. Thus an hypothesis may be false though workable. And the scientist who lauds a theory, because it leads to verifiable predictions, has been offered bread, but has chosen a stone. He has turned his back upon the most valuable attributes of his theory. The true is always workable, but the workable is not always true.

The inductive process whereby an hypothesis after repeated veri-

[1] This paper was read at Professor Royce's seminary, January 18, 1916.

[2] See this JOURNAL, Vol. XIII., p. 236.

[3] S. L. Bigelow, *Theoretical and Physical Chemistry*, 1914, p. 140.

fications becomes a law is an evolutionary process in the advancement of learning. The opinion which is possibly true rises to the plane where it is probably true. Here is improvement—and growth of knowledge. But, for an hypothesis to become a theory—speaking strictly, there is no such thing. The hypothesis, however workable it may be, is or is not already a theory. Some one notices that these are the conditions without which the phenomena could not be, and—lo, we see that what we had taken all along for an hypothesis is really a theory after all—and it's true. What is more natural than that it should have led to verifiable predictions. The multiplication of instances of successful prediction demonstrates more and more the workable character of the hypothesis, but it proves nothing; and if it happens that it is a theory already that we are considering, our ability to predict from it facts that we already know adds not one jot to our knowledge.

I. Some Pragmatic and Other Aspects of the Kinetic Theory of Gases

In 1738 Daniel Bernouilli came forward with the hypothesis that gases consist of particles moving about with great rapidity, hitting against one another and against the walls of the containing vessel, bouncing off again, like elastic balls, to continue their movement, with unimpaired velocity, in a new direction. According to this hypothesis the pressure of the gas would be due to the many impacts of the many particles upon the walls of the containing vessel; and, consequently, the pressure might be increased either by increasing the number of particles in the given volume or by increasing the velocity with which they moved.

More than a century later, in 1851, Joule showed that one of the consequences of this hypothesis is Boyle's law—and that law is known to be genuine. The successful prediction from this hypothesis of verifiable consequences brought it again to the attention of scientists. It was taken up by Krönig and Clausius in 1857 and has since been developed by many others in its various ramifications.

Joule's reasoning was essentially as follows: Suppose that the quantity of gas contained in a cubical container, whose sides are l centimeters long, consists of n particles, each of the mass m, and each moving with an average velocity V. From a consideration of the parallelogram of forces, it follows that the force of impact of the particles striking against any side of the cube is the same as it would be if one third of all the particles ($\frac{1}{3}n$) were moving in a direction perpendicular to that side. One particle will require l/V seconds to move from one side to the opposite one, and will therefore strike the given side $\frac{1}{2}(V/l)$ times per second. The momentum

of the moving particle is mV and on bouncing away from the side after the impact it has a momentum quantitatively the same, but of opposite sign. Its change of momentum then is $2mV$. Thus the total change of momentum of $\frac{1}{3}n$ particles striking the side $\frac{1}{2}(V/l)$ times per second will be $[\frac{1}{2}(V/l)](2mV)(\frac{1}{3}n) = \frac{1}{3}(nmV^2/l)$. This is the total pressure exerted on any side. Since the area of the side is l^2, the pressure per unit area will equal $\frac{1}{3}(nmV^2/l^3)$. But l^3 equals the volume v of the cube. Therefore $p = \frac{1}{3}(nmV^2/v)$ or $pv = \frac{1}{3}nmV^2$. For the gas under consideration n, m, and V are constant values; then the total of everything on the right of the equality sign is constant, and $pv = K$—which is the expression of Boyle's law. The ingeniousness of the reasoning demands admiration.

Joule's reasoning is brought forward very frequently as evidence in favor of the kinetic theory. Aside from the mathematical part it amounts to this:

1. If the kinetic theory were true, Boyle's law would be a physical necessity.

2. Boyle's law is genuine (instances of it are fact). Would any one think of concluding from these propositions, "Therefore the kinetic theory is true?" Stripped of all concrete significance, the propositions become

1. *A* implies *B*,

2. *B* is true;

and it certainly does *not* follow that *A* is true. The true implies the true, the false implies the true, and the true may be implied by either the true or the false. No judgment upon the truth or falsity of *A* is justified. Yet *A* works.

My only excuse for discussing these truisms of logic is their bearing upon pragmatism. The kinetic theory as used by Joule *worked;* its pragmatic value for him could be defined as its pointing toward verifiable conclusions; its *working* lay in the fact that it implied the true. Yet we know that the true may be implied by the false. Science, therefore, must refuse to accept the pragmatic criterion. *Why should we satisfy ourselves with hypotheses which imply the observable, when we can get theories which the observable implies?*

Does the fact that *A* implies *B*, while *B* is true, render *A* *probably* true? One would be tempted at first sight to say that it does not. Would the fact that *A* implies *B*, and *C*, and *D*, and *E*, etc., where *B*, *C*, *D*, and *E*, etc., are true, render *A* probably true? One begins to become suspicious. The fact pointed out by Joule, that the kinetic theory implies Boyle's law, is one instance of the workable character of the kinetic theory. It happens moreover that a

great many gas laws and observable properties of gases may be predicted from the kinetic theory. These are further instances of its workableness—and altogether there is quite an array of "fair samples." The induction is justified that the kinetic theory is probably always workable. If *A* implies *B* and *B* is true, then in this case *A works*. Suppose that in other cases where *A* implies *C, D, E, F,* and so on (where *C, D, E,* and *F* are "fair samples") it is still found to *work,* then it is probable that *A* universally *works*. That is, it is probable that *A* always implies the true. But that which *always* implies the true is itself true. Then it is probable that *A* (always implies the true and) is true.

In a word, then, "fair sample" cases in which the results predicted from an hypothesis are verified render the hypothesis probably true.[4] Still it is going rather far to hold up the kinetic theory as probable on the strength of the single successful prediction of Joule. Yet there are so many similar cases that it is indeed highly probable.

The kinetic theory certainly provides an excellent working explanation of a large group of phenomena. But isn't it really more than a plausible hypothesis? Isn't it a theory, in that it is, in the main, a statement of the conditions without which the phenomena could not be? Isn't it true?

There seems little reason to doubt that Joule looked upon his own explanation of the mechanics of gas-pressure as an hypothesis adapted to the fact. Yet at the time in which Joule lived there were sufficient experimental data available to justify his conclusion as theoretically true. Facts which had already been observed implied his explanation. The physical and chemical behavior of gases implied Avogadro's Theory, namely, that equal volumes of gases under the same conditions of temperature and pressure contain equal numbers of molecules (or what may be called "physical units"). The fact of the existence of substances of the same composition, yet palpably different, implied that the matter of which they were made consisted of separable particles. Then certain gases, specifically those which can be used in synthesizing isomeric substances, definitely do consist of particles, and equal volumes contain under the

[4] This seems a great advance over such inductions as "all swans are white," "all men are mortal." For, here, by the examination of one set of things we confer probability on another set. This is a theorem of what might be called "the calculus of fair samples." All the theorems of this calculus would not lead to probabilities. Another was developed in the paper referred to above. Many others are hidden, I imagine, within masses of scientific data. Such a calculus, if complete, would be the complement of the algebra of logic—for within the two our manner of knowing all things, except the metalogical, would be comprehended.

same conditions equal numbers of molecules, and the molecules themselves contain each a definite number of atoms. Joule knew that gases diffuse into one another to form homogeneous mixtures, that a heavy gas diffuses upward into a lighter one, and that, when the diffusion is complete, there is never any tendency on the part of the heavier gas to settle out again. Had he reasoned that these things could not be unless the particles of which gases consist are in constant movement, hitting against one another and bounding off again with unimpaired velocity, his conclusions would have had the truth which attaches to a theory. Graham's law states that the rates at which gases diffuse are inversely proportional to the square roots of their densities. This means, since equal volumes contain an equal number of particles, that the rates of diffusion are inversely proportional to the square roots of the masses of the particles. Had Joule reasoned that this law could not be unless the velocities of the particles of gases (at any temperature) were inversely proportional to the square roots of the masses of the particles, his conclusions, again, would have had the truth of a theory. Since the kinetic energy of a moving mass is proportional to the mass and to the square of the velocity (proportional to MV^2), Joule might well have reasoned that all these facts could not be unless the average kinetic energy of a particle of one gas, at a given temperature, were equal to the kinetic energy of any particle of any gas, at the same temperature. All these facts were well known in 1851, and Joule could have demonstrated to anyone who had the patience to listen—and patience is needed, I fear—that the kinetic theory of gases is the necessary implication of the observed behavior of gases. As such it is true.

In his paper of 1851 Joule actually calculated the average velocity[5] of the particles of certain gases at a definite temperature. It is interesting to notice that the velocities calculated by Joule are actually *true* velocities. Their numerical exactness is limited only by the precision of the physical measurement of the quantities involved in the mathematical calculation.

The viscosity of gases, at any given temperature, is independent of the pressure upon them. If gases are homogeneous throughout, the effect of an increase of pressure would be to increase the quantity of stuff in a given volume, and consequently to increase the viscosity. If gases consist of moving particles, the effect of an increase of pressure would be to increase the number of particles in a given

[5] In the equation given above p and v can be measured directly; n and m can not be measured, but their product, nm, the number of the particles times the mass of the average individual particle, is equal to the total mass of the gas under consideration. This can be measured, and V calculated.

volume, but the velocity of the particles would not be altered, and the viscosity would remain unchanged. We may say that unless the kinetic theory were true, in the main, the effect of pressure on viscosity could not be as it is observed. Hence the kinetic theory is true. Lord Rayleigh says that the viscosity of gases is, to his mind, the best evidence that there is for the kinetic theory. Professor Richards, who reports the conversation, remarks that this is interesting in that it shows the point of view of the physicist, but for the chemist there are also many other considerations which point toward the kinetic theory. Some of these considerations have been indicated.

The evidence which has thus far been presented for the truth of the kinetic theory is, to say the least, complex and technical. A simple and beautiful experiment has been performed very recently by Dr. Wood, of Baltimore, so simple that its implications would be evident to children in grammar school—and to this experiment the main tenets of the kinetic theory stand in the position of the *conditiones sine qua non*. The confessed object of the experiment was to obtain a gas in which all the molecules were moving in one direction. This result was to be attained by removing all the molecules which happened to be moving in other directions. Mercury vapor was admitted at the end of an evacuated cylindrical glass vessel, the vessel being submerged in liquid air. Since the particles of the incoming vapor were moving about in all directions and colliding with one another, many of them found their way at last to the sides of the cylinder. When they struck the sides of the cylinder, the cold liquid air removed most of their heat, they did not bounce off again, but remained condensed upon the cold glass surface. The rest of the particles, then, were moving in more or less straight lines through the length of the cylinder. At the center of the other end of the cylinder there was a small hole leading into a spherical glass chamber. The mercury condensed in a droplet *opposite this hole*, but nowhere else upon the walls of the spherical chamber. It seems to me that much genius is needed in the man who would devise a single simple experiment which means as much as this one. For any one can see that this experiment *could not be* unless the main tenets of the kinetic theory were true.

The kinetic theory is the rock upon which much of thermodynamics, and of physical chemistry, and of physics is built. That this theory, in the main, is as true as any bit of experience is a highly important matter. It is for that reason that I have selected it for the present discussion—in spite of the fact that the data upon which it is based are not of the most familiar kind. It has appeared that it possesses a truth wholly beyond that inherent in its prag-

matic workings. So it is with all real theories. Theory and hypothesis are wholly unlike. The logical nature of this contrast will appear more distinctly from a consideration of simpler cases. This consideration will serve also to show again that the tendency of scientific men to theorize is not devoted wholly to the production of highly important theories which form the basis for several sciences at once, but that it leads to little pieces of new information (really theories, but seldom called so)—tucked away oftentimes in the corner of some special science, but having that which far more important hypotheses do not have—namely, truth.

II. Other Instances of Theory

Although sun-spots had been observed with the naked eye before the time of Galileo, his telescope permitted him to make more extended observations upon them. With the new information at hand, he straightway began to theorize. The observation that the spots appeared to move across the face of the sun, coming to him upon the heels of his discovery of the satellites of Saturn, led him to suppose at first that their appearance was due to planets revolving near to the sun and appearing as black spots against its luminous disc. Presently he noticed that occasionally one large spot divided itself into several, and occasionally several combined to form one. The spots varied in degree of darkness. As they approached the edge of the sun's disc they appeared to contract in breadth, without losing in length. Finally, it sometimes happened that a number of spots of varied size appeared in a certain order, and then, after a period of disappearance, they reappeared again in the same order. Galileo gave the true explanation of these phenomena: the spots were not planets intercepting the sun's light, they were on the sun itself and the sun was rotating about an axis. At this distance it is evident that the explanation of Galileo is a *theory*. Unless the sun rotates about an axis, and unless the spots are on the sun, these things could not be as they are observed. Here is no mere working hypothesis. *This* explanation is implied by the observations.

The situation becomes still more interesting when there are several hypotheses, all equally workable for the phenomena under consideration. Sooner or later there comes a time when an observation or critical experiment is made which renders one or more of the hypotheses, maybe all but one of them, now unworkable. The surviving hypothesis still "applies"—subject always to the possibility that a further situation may arise which will render its abandonment or revision necessary. As a matter of logic it *may happen*, as a matter of fact it often *does happen*, that the new observation is such that it not merely renders some of the hypotheses unworkable,

but actually implies or presupposes some other. That which is so implied is, of course, as true as the observation which implies it. It is no guess. It is a theory.

The theory of the manner in which coral islands are formed is an admirable case for the present discussion. There are three kinds of coral islands: "Fringing reefs," which lie along the shore of a mountain which rises out of the sea, the commoner type of coral reef enclosing in its more or less circular form the mountain-island, separated from it on all sides by a lagoon of placid water, and finally, the "atol," more or less circular and enclosing no island. It is known that corals grow only in water of less than a certain depth. Therefore, one may say—however these reefs were formed,—they certainly had their beginnings in shallow water.[6] Darwin offered the explanation that the coral growth began as a fringe upon the shore of the mountain-island; slowly, through geologic ages, the island subsided beneath the ocean, and new growths of coral arose upon the older coral formation now deeper below the surface. Thus, after the mountain had subsided for a certain distance, the result would be a reef encircling the island and separated from it by a considerable space of water. If the island sank entirely below sea-level, the result would be an atol. This explanation worked. It explained the general circular form of the coral islands; it fitted with the fact that corals do not grow in deep water.

There were other explanations. One supposed that the action of the waves washed the island away, either entirely, or all but the central part, and that the corals grew up afterwards upon the edge of the resulting shoal area. Another supposed that the corals grew first upon the coast of the island, and that the island was afterwards washed away wholly or in part by the action of rains or of the sea. It is to be noticed that these hypotheses also explained how the three kinds of coral islands might have been formed.

Some years later the observation was made in the light of which one of these explanations is seen to be a theory. A geologist, having climbed one of the volcanic mountain-islands of the Hawaiian group (the island was surrounded by a coral reef) noticed that the sides of the mountain were heavily furrowed. The rain-water running

[6] Throughout this part of the discussion there are two assumptions: (1) That time was as it is now, that temporal processes extend backward farther than we can grope or conjecture; and (2) that the laws of nature (whatever they may happen to be) were effective in this past time even as they are now. I shall not attempt here to defend these assumptions because they are not assumed for the discussion of *theory*, but rather for the discussion of something geological—without them there could be no question of geology. To deny these assumptions is to deny the right of geology to any truth whatever. Grant them, and geology may have its theories just as the other sciences have theirs.

down the mountain had worn valleys in the sides. Wherever one of these met the sea there was a bay. Now the rain-water, in coursing down the mountain, had carried with it large quantities of solid material, else it could not have worn away these valleys. If the mountain-level or the sea-level were not changing, then this material, which was poured out into the water near the shore, would have deposited and formed a delta. Actually there were no deltas; observation showed just the opposite thing, namely, bays. There could be no bays where the valleys met the sea *unless* the island were sinking below the sea-level at a rate faster than deltas could be deposited: therefore the island was sinking. Such a statement is logically of the nature of a theory.[7]

Let us reconsider the whole matter, while the various considerations are stripped of their concrete significance. We have here three hypotheses, H_1, the Darwinian explanation, H_2, and H_3, the other explanations which have been mentioned. All of these work for certain phenomena, A_1, A_2, and A_3, namely, the formation of the three types of coral island. They *imply* these three types, and the three types are observed. That is,

$$H_1, H_2, H_3 \text{ imply } A_1, A_2, A_3;$$
$$\text{and } A_1, A_2, A_3 \text{ are observed.}$$

The observation of Dana, upon the rain-valleys of the mountain-side and the bays upon the shore, A_4, was an observation of phenomena which are implied by H_1, but not by H_2 and H_3 (for, if H_2 or H_3 were true, there would be deltas). H_2 and H_3 then drop out of the running. They no longer work. There is left H_1, the sole-

[7] Whether King Arthur's sword sank beneath the waters of the lake or the waters rose above King Arthur's sword is a verbal question. Here too, in discussing the manner of formation of coral islands, we may, if we wish, disregard the question of whether the mountain absolutely sinks or the sea absolutely rises. For geology, of course, this is an important question, but it does not bear directly upon the mode of formation of coral islands. This question, too, perhaps, is, or will be, capable of a theoretic answer.

The original hypothesis as brought forward by Darwin insisted upon the *absolute* subsidence of the mountain. The American, Dana, made the observation of the embayments at the foot of the valleys, but he did not appreciate its significance. The *relative subsidence theory* is recent. For the facts in this part of the discussion I am indebted to Professor W. M. Davis, of Harvard.

Some of the theories of science are rigidly implied by the facts. Some are implied only if certain laws of nature (really probable) are assumed to be true. In many cases hypotheses which work by implying the verifiable imply the verifiable only by virtue of certain laws which are assumed to be true. So, whether the implication be rigid or be of this inductive kind, the distinction between theory and hypothesis is still genuine; and, though several of the implications in the foregoing are of this latter kind, no harm is done, so long as the confession is made.

surviving successful hypothesis. It implies all of the relevant facts which have been observed so far. That is

H_1 implies A_1, A_2, A_3, A_4;
and A_1, A_2, A_3, A_4 are observed.

A_4 appears here as a crucial observation. It decides against certain hypotheses in favor of some other. That, it seems to me, is as far as pragmatism can get. But it is not yet a complete statement of the logic of the case. The fact that there is a bay where the rain-washed valleys of the mountain meet the sea implies that the mountain is slowly subsiding below the sea. . . .

A_4 implies H_1;
and A_4 is observed.

Clearly then, if A_4 is true, H_1 is true also. The true implies only the true. We are as sure of H_1 as we are of A_4. H_1 is as true as any bit of experience. It is a theory.

The more cases there are in which an hypothesis works, provided the cases are always "fair samples," the more *probable* it becomes that the hypothesis is true. That is the best we can ever hope for from any process which consists wholly of guessing and verifying. But guessing is not theorizing: and for the scientist, the great value of the "method of multiple working hypotheses" lies in the fact that, the more hypotheses he has, the more likely it becomes that one among them will some day be implied by some new experiment or observation. The one so implied is a theory—and it is as true as the corresponding piece of experience. There seems to be many such theories in science.

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THE WILL TO LIVE

ONE of the important facts that psychology contributes to human wisdom is this—that any stimulus, even an unpleasant one, is usually attractive when it is not too strong. This fact is generally dropped into a footnote, as though it were an amusing though negligible accident, but it is quite generally agreed to. I have noticed it recently in works so wide apart as Washburn's *The Animal Mind* (p. 43) and McDougall's *Social Psychology* (p. 150). Washburn says, speaking of Jennings's experiments with the amœba:

"The significance of the positive reaction is harder to determine. It seems to be given in response not to a special kind of stimulus,

but to a mechanical or food stimulus [*i. e.*, practically any perceptible stimulus] of slight intensity."[1]

McDougall says:

"The intrinsic feeling-tone of the emotions seems to follow the same rule as that of the sensations, namely, that with increase of intensity of the emotion pleasant tends to give way to unpleasant feeling-tone; so that, while at moderate intensities some are pleasant and others unpleasant, at the highest intensity all alike become unpleasant or painful; and, perhaps, at the lowest intensity all are pleasant."

"Thus fear," he goes on to say, "at low intensity does but add a pleasurable zest to any pursuit, as we see especially clearly in children, sportsmen, and adventurous spirits generally; whereas at high intensity it is the most horrible of all experiences."

It will be remembered how James shocked our sensibilities by telling us that we enjoy the smell of a skunk that is not too near. "Everyone," he says, "who has a wound or hurt anywhere, a sore tooth, *e. g.*, will ever and anon press it just to bring out the pain. If we are near a new sort of stink, we must sniff it again just to verify once more how bad it is."

These statements are all strongly confirmed in my experience and observation. I notice that children always want to do whatever they see any one else do, and as it is old-fashioned to say that they have an "instinct of imitation," we shall have to say that they have a thirst for experience which is very general. They want to "see what it feels like," and after they have seen what it feels like, then they want to feel it again and more strongly. They want to increase and intensify their experience. Every one of us, I suspect, could remember instances in which he courted sensations that are instinctively abhorred. He courted *abhorrence.*

All this is in my mind just now, because I am editing a magazine which is illustrated by a group of artists to many of whom "beauty" is anathema, and loveliness, or gracefulness, or the decorative, or any of those humble ideals of our youth are regarded as exceedingly

[1] Jennings himself (*Behavior of the Lower Organisms*, p. 116), cites the observations of Pearl on the behavior of the flatworm, *Planaria* (whatever that is), to the following effect:

"Sometimes individuals are found which for a brief period (two or three hours) seem in a much more active condition than usual. They move about rapidly, but do not conduct themselves like the excited individuals. As they move they keep the anterior end raised and wave it continually from side to side as if searching. Specimens in this condition react to almost all mechanical stimuli, whether weak or strong, by the positive reaction, turning toward the point stimulated. Experimentation failed to show that this condition was due to hunger."

rustic and out-dated. These artists seem both healthy and happy, and subtracting their dogmatism, I see nothing to distinguish them from others except that they are more like children. They have never been taught to fear the censorship of some one who proclaims a social standard of good taste. The standard they set for themselves, as nearly as I can formulate it, is that their drawings should always have *some pronounced quality*. In other words, their positive tropisms are a little indiscriminate. They are attracted by stimulation in general.

And so in some degree are we all. We are like the amœba. Its negative tropisms are specialized; they appear only where they are necessary to survival. But its positive tropisms are not merely something developed through utility—they were a fundamental general attribute of its nature.

This means to me that life has an interest in living. And I think this too obvious fact is being overlooked not only in the sciences, but in the logics and philosophies of to-day, with results that will appear somewhat absurd to-morrow.

It was natural that in an age and nation so devoted to business as ours, a practical conception of universal life and mind should arise and dominate the field of science. And beyond question the fruits of that conception will be lasting. But in time we shall recover from the idea that everything that happens, from tropism to truth, is a practical adaptation. We shall recover so completely, I hope, that no mystical or metaphysical affirmations will have to take its place. We shall simply find ourselves not making that assumption any longer, but spontaneously conceiving organisms and intellects as rather more adventurous than they used to be.

We shall find not only that experience as such is welcome to life, but that life of its own accord goes in search of experience. That "general motility" which Jennings has to add to the specific reactions, in writing the biography of lower organisms, will, if separately dwelt upon, supply a standpoint from which life can be viewed as fruitfully as from the standpoint of adaptation to stimuli. We are not merely trying to adapt ourselves in order to stay alive, but we are trying also even more energetically to live. Everything we do and think is not a reaction; a great deal of it is action. The "Behaviorist" is not so much to be condemned for his refusal to observe or consider "states of consciousness," as for his totally inadequate view of what he does observe and consider. The interaction of organism and environment is for him carefully divided into reflex arcs, all operating in one direction. A stimulus to the end-organ, a commotion in the central-nervous system, then a response in the muscles—that is the whole story of life in his laboratory. But life

interflows with reality in full circles. We do things not only because we have a sensation, but also in order to make a sensation. And so do the most elementary organisms. Any rubber ball can react, but it requires life to act. And life does act.[2] It seeks experience. That is what we shall learnedly discover when we are passing out of the age of the sovereignty of business.

In the life of mind this spontaneous seeking, and this pleasure in stimulation as such (which I have called elsewhere a "poetic impulse"[3]) appears in two forms. It appears first as a tendency to linger on and develop the imaginary and emotional accompaniments of thought. There is much vivid imagery in the minds of some people, and they enjoy it. They enjoy it almost as much as they enjoy hearing the unluckier people assert that such imagery does not exist. But even for these unlucky there are ways of remembering and imagining which create quite a commotion in their bodies, and they enjoy that. They enjoy recalling experiences for their own sake. And that gives rise to poetic speech.

Poetic speech is speech in which the quality of the experiences that are mentioned is dwelt upon to a greater degree than the meaning requires. Writers of esthetics usually ask themselves why people compose poems. But that is a question which can not be answered in general. People compose poems for many different reasons—because they want to have them, they want to show them to somebody, they feel bad, or they feel good, they desire praise, or they are expected to compose them. The question is neither answerable nor important. An important question that can be answered is, *why do so many people when they have something to say, inevitably say it poetically, at a considerable loss of time and effort?* Why are we all delighted with the conversation of such people? And the answer is, *because we all have an underlying general interest in experience, whether pleasant or unpleasant, and we like to have it touched off to our imaginations as we go along.* We have a yearning that is not hunger, nor love, nor curiosity, nor any of the specific instincts, but is in fact a concrete manifestation of that will-to-live which occupies so important a place among abstractions.

This "will to live" appears also in forms of language which are not so colorful as what we call poetry. In philosophy itself, much that is puzzling may be explained by confessing that life has an

[2] This position is not meant to, and does not, deny the value of a mechanistic view of the world. It is quite possible to conceive that the tendency to seek experience and activity for their own sake was imparted to the cells of life at some time by some causes. The point is to recognize that, as they are known to us, they have this tendency; they are not inert until stimulated by their environment as it is the fashion of mechanistic behaviorists to conceive.

[3] *Enjoyment of Poetry,* Chapter I.

interest in living. I am sure it is beyond the scope of the reflex-arc to explain Mr. Bertrand Russell's interest in laying out months and years of his life in order to prove that he is not quite sure whether an external world exists, but he believes that it can be proven either that a little of it does or does not exist, if we all keep at it for a long time! I fail to find anything practical or anything adaptive in Mr. Russell's intellectual tropisms. They too have the character of *general motility*.

Mr. Russell's book[4] (after I got through its title) was quite absorbing to me. And although in the middle of it I read and agreed with John Dewey's demonstration[5] of the fact that the author could not fruitfully *even ask the question* of which he was discussing the answer, my interest in his discussion continued to the end. It did not seem to matter whether the question of the existence of the world "meant anything." I always approach these metaphysical books in the mood of the gentleman who said: "Well, how do you do?—Not that I give a damn, but just to keep up the conversation." And it is my earnest belief that John Dewey's demonstration of the impossibility of asking that question without assuming an answer, can be applied to every treatise upon any question strictly to be called metaphysical. We all darkly know that we are dishonest when we write metaphysics. We make these subtle deceptive assumptions for the sake of getting a question asked, and we discuss the question in a learned language whose very structure involves an answer, because we are interested in the intellectual adventure. If we did this playfully, and regarded our books of metaphysics as we regard a game of chess, we need not hide those assumptions from our consciousness; but a great part of the emotional content of the adventure would be lost. It is an adventure among entities, absolutes, universes, not merely among ideas in our minds. It is our serious life that wants to be playful, and in order to carry its seriousness into this field of play it has to delude itself with a pretense of fundamental rationality that is impossible there.

Metaphysics is at least sincere and correct in asserting its complete irrelevance to any practical problem. It is a holiday excursion. It is life living; not life preserving itself. And though its results may not partake of the nature of truth—truth may belong wholly to the sphere of adaptation—they have their own value. They and those terrific paths of rational discipline that lead to them are a part of the wealth of being. And this fact will give us no difficulty,

[4] *Our Knowledge of the External World as a Field for Scientific Method in Philosophy.*

[5] "The Existence of the World As a Problem," *Philosophical Review*, July, 1915.

even when the outcome is as piecemeal and meaningless and barren of beauty as Mr. Russell's method makes it, if we recognize that organisms seriously thirst after experience in general. There is another kind of value in the world besides truth, and beauty, and the good, and that is life itself. And no one will deny that metaphysics, even of the new unsoulful kind, is a very strenuous form of life.

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REVIEWS AND ABSTRACTS OF LITERATURE

A Budget of Paradoxes. AUGUSTUS DE MORGAN. Edited by D. E. SMITH. Chicago: Open Court Publishing Company. Pp. viii + 402, 382.

Modern science has been built up by slow and laborious processes, and to achieve even a moderate understanding of it requires more labor than the great majority of the literate are willing to exert. This prepares the ground for the brood of impatient romantic souls that are eager to substitute some brilliant scheme of their own for the monotonous régime of established science. In the end these seekers after the philosopher's stone, social and physical panaceas, and other romantic get-rich-quick schemes are eliminated by their own natural sterility. But in spite of this sterility they are an interesting brood and their natural history is worthy of serious study. Such a study might contribute greatly to the understanding of the sociology, psychology, or pathology of the intellectual life. So far as I know no such study or special investigation has ever been undertaken. De Morgan's *Budget of Paradoxes,* however, is a wonderful source- or case-book for such a study. It does not go into the etiology of the subject, but supplies us with a rich museum of intellectual oddities. But one may read and enjoy these two volumes quite apart from any technical scientific interest. De Morgan was one of those rare minds that can combine a wealth of accurate learning with originality of insight and genuine wit, and the reading of these volumes is bound to evoke a feeling of gratitude to the editor and publisher for making the book more available to the general public.

The bulk of these two volumes is devoted to the mathematical heretics, the squarers of the circle, trisectors of the angle, and subvertors of the Newtonian theory of gravitation. But there is a great deal of good sense about science generally. Especially worthy of mention are the remarks on the snobbery of men of science (*à propos* of the F.R.S. and other expensive titles, pp. 25 and 30), the merciless showing-up of the myth about induction and about Bacon as the

founder of modern scientific method (I., pp. 75–79), and the answer to the theologian's appeal that the probabilities of science should not be used to attack the certainties (!) of theology (I., p. 316).

The following may give those unfamiliar with this book a taste of the style: "I look upon all the churches as fair game which declare [*cf.* the Athanasian Creed] that *absque dubio in æternum peribo:* not for their presumption towards God, but for their personal insolence towards myself. I find that their sectaries stare when I say this. Why, they do not speak of you in *particular!* . . . But I hold with the schoolmen that '*omnis homo est animal*' in conjunction with '*sortes est homo,*' amounts to '*sortes est animal.*' . . . If this be not conceded, then I retort in their own manner, 'Whosoever would serve God, before all things he must not pronounce God's decision upon his neighbor. Which decision, except every one leave to God himself, without doubt he is a bigoted noodle'" (II., pp. 32–33).

For an analysis of the differences between the logic of Protestant and that of Papal Christianity, I know nothing in all literature comparable to De Morgan's parable of the two milk dealers (II., pp. 33–36). The conclusion is still applicable in this day of friendly alliance between philosophy and theology: "The theologian writes a philosophy which seems to tell us that the New Testament is a system of psychology; and the philosopher writes a Christianity which is utterly unintelligible as to the question whether the Resurrection be a fact or a transcendental allegory. What between the theologian who assents to the Athanasian denunciation in what seems the sense of no denunciation, and the philosopher who parades a Christianity which looks like no revelation, there is a maze which threatens to have the only possible clue in the theory that everything is something else, and nothing is anything at all" (II., p. 36).

The editor has industriously ransacked the printed encyclopedias for biographical facts concerning the diverse worthies mentioned by De Morgan. But some readers would have been more grateful to him if he had been able to discover the name of the logician, friend of De Morgan, who addressed the latter thus: "Next to the man who forms the taste of the nation, the greatest genius is the man who corrupts it. I mean, therefore, no disrespect, when I say that I have hitherto always considered you as a great logical heresiarch." It may, however, be too much to expect that the reading of these volumes might induce some to become familiar with De Morgan's acute and fertile logical "heresies" embodied in his *Formal Logic* and in his memoirs in the *Transactions of the Cambridge Philosophical Society* —of which no mention is made anywhere in these two volumes.

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A Text-book on Logic. ARTHUR ERNEST DAVIES. Columbus, Ohio: R. G. Adams and Company. 1915. Pp. 601.

The style of the author is simple, easy, and entertaining. As the reader proceeds he is overtaken with the feeling that every aspect of the topic under discussion is to receive full consideration. It is evident that Professor Davies is master of his subject and is bent on clearing up many of the abstruse problems of logical doctrine. The book is free from bombast and scholastic assumption, and the reasoning is logical and conclusive. The work is written in the spirit of the thinker and not the teacher, therefore system and teachableness are sacrificed on the altar of comprehensiveness. From the viewpoint of induction and deduction the book is well balanced. There seems to be no attempt to relate the subject of logic to the theory and practise of education. It is not a text for the beginner, but should fill a felt need in the more advanced college work.

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JAMAICA, NEW YORK CITY.

JOURNALS AND NEW BOOKS

THE AMERICAN JOURNAL OF PSYCHOLOGY. July, 1916. *The Renewal of French Thought on the Eve of the War* (pp. 297–314): ALBERT SCHINZ. – The artists, poets, writers, and philosophers of France are turning away from rationalism or realism toward idealism and a genuine religiosity. This movement is ridding France of dilettantism and is restoring to her in a religious and philosophical way her more abiding ideals. *The Number of Observations upon which a Limen May Be Based* (pp. 315–319): E. G. BORING. – The author opposes Fernberger who in lifted weights insisted that at least 50 tests should be made. The claim is made that fewer cases can often mean as much or more, depending on the problem and circumstances. *The Gesture of Affirmation among the Arabs* (pp. 320–323): S. S. GEORBE. – H. Petermann in his *Reisen im Orient* states that the Arabs in affirmation shake the head as we do in showing negation. It is shown that such movements as now practised and as shown in literature are essentially the same as ours. The shaking motion was probably an accompanying gesture of determination, pride of execution, or the rising to the occasion. *Positive After-Images of Long Duration* (pp. 324–334): P. F. SWINDLE. – Experiments with an owl and a cockatoo indicate that they have positive after-images of long duration. With the proper illumination and presentation positive after-images of long duration can be induced in

the human eye. *A Simple Daylight Photometer* (pp. 334–340): C. E. FERREE and GERTRUDE RAND. – Description of a simple photometer for laboratory use. Quickly gives readings for intensity of daylight when the same illumination is desired. The photometer can also be used for measuring illumination in absolute units. *The Nature of Recognition Memory and of the Localization of Recognitions* (pp. 341–362): MARGARET HART STRONG and EDWARD K. STRONG, JR. – The factor of localization in recognizing is brought about in the ease with which the nervous discharge takes place over the associational paths involved. *Psychoanalytic Studies of Genius* (pp. 363–416): LUCILE DOOLEY. – A collection of epitomes of psychoanalytic studies of genius. The series includes the following: S. Freud, *The Delusion and the Dreams in Jensen's "Gradiva";* S. Freud, *A Childhood Memory of Leonardo da Vinci;* Karl Abraham, *Giovanni Segantini;* Ernest Jones, *Andrea del Sarto's Art, and the Influence of his Wife;* Ernest Jones, *The Problem of Hamlet and the "Œdipus-Complex";* Alice Sperber, *On Dante's Unconscious Soul-life;* J. Sadger, *The Love-life of Nicolaus Lenan;* J. Sadger, *Heinrich von Kleist;* Otto Kaus, *The Case of Gogol;* Max Graf, *Richard Wagner in the "Flying Dutchman";* Otto Rank, *The Lohengrin Saga;* Ludwig Jekels, *The Turning Point in the Life of Napoleon I;* Ernest Jones, *The Case of Louis Bonaparte, King of Holland;* Karl Abraham, *Amenhotep IV. Echnaton. Psychoanalytic Contribution to the Understanding of his Personality and of the Monotheistic Aton-cult;* Oskar Pfister, *The Piety of Ludwig von Zinzendorf;* Oskar Pfister, *The Hysteria and Mysticism of Margareta Ebner;* Georg Lomer, *Ignatius Loyola, from Erotic to Saint;* Edward Hitschmann, *Schopenhauer. Attempt at Psychoanalysis of the Philosopher: and some other studies. Minor Studies from the Psychological Laboratory of Cornell University:* E. B. TITCHENER and H. P. WELD. *Mechanical vs. Manual Stimulation in the Determination of the Cutaneous Two-Point Limen* (pp. 417–419): M. CARNES and L. C. SHEARER. – Mechanical application is advantageous because of a little greater scientific accuracy. *On Memorizing with the Intention Permanently to Retain* (pp. 420–426): F. P. BOSWELL and W. S. FOSTER. – The intention is effective in its influence on the mode of learning. *Some Uses of Artificial Daylight in the Psychological Laboratory* (pp. 427–429): A. J. BROWN. – Artificial daylight produced by means of Gage's glass proved very satisfactory in laboratory and class-room color work. *On the Psychological Response to Unknown Proper Names* (pp. 430–434): G. ENGLISH. – The psychological response to unknown proper names depends on the imaginal type, associations, circumstances, and other highly variable factors. *A Note on the Compensation of Odors* (pp. 435–436): E. B. TITCHENER. *Book*

Review (pp. 437–439) S. T. ORTON: Knight Dunlap, *An Outline of Psychobiology. Book Notes.*

Hamilton, G. V. A Study of Perseverance Reactions in Primates and Rodents. Behavior Monographs, Vol. 3, No. 2. Edited by John B. Watson. Boston: Henry Holt and Company. 1916. Pp. 65.

Lozano, Fernando. La Raiz de la Guerra y El Fundamento de la Paz Definitiva. Madrid: Imprenta Espanola. 1916. Pp. 30.

Miner, Maude E. Slavery of Prostitution: A Plea for Emancipation. New York: The Macmillan Company. 1916. Pp. xi +308. $1.50.

NOTES AND NEWS

HUGO MÜNSTERBERG

1863–1916

HUGO MÜNSTERBERG was born in Danzig, Germany, June 1, 1863. After graduating from the Gymnasium of his native city in 1882, he studied philosophy, natural science, and medicine at the Universities of Geneva, Leipzig, and Heidelberg, from 1882 to 1887, obtaining the Ph.D. degree at Leipzig in 1885, and the M.D. degree at Heidelberg in 1887. He entered upon his academic career as *Privat-Docent* in philosophy at the University of Freiburg in 1888, and was promoted to the rank of assistant professor in 1891. In 1892 he was called to Harvard University as professor of psychology and director of the psychological laboratory; he occupied the chair at Harvard continuously until his death, but spent a year abroad as Harvard Exchange Professor at the University of Berlin (1910–11), where in addition to his professorial duties he organized and directed the *Amerika-Institut* of the German government. Professor Münsterberg was vice-president of the International Congress of Arts and Sciences in St. Louis in 1904; of the International Psychological Congress at Paris, 1902; of the American Association for the Advancement of Science, 1906; and of the International Philosophical Congress at Heidelberg, 1908. He was president of the American Psychological Association in 1898, and of the American Philosophical Association in 1908. This career of remarkable activity and achievement was brought to a sudden close on December 16, 1916, when Professor Münsterberg was stricken while addressing a class-room of students at Radcliffe College.

Professor Münsterberg was the author of numerous papers and books on psychological, philosophical, and other topics. His pub-

lished works include the following: *Die Willenshandlung,* 1888; *Beiträge zur experimentellen Psychologie,* I.–III., 1889–92; *Verse* (Hugo Terberg), 1897; *Psychology and Life,* 1899; *Grundzüge der Psychologie,* I., 1900; *American Traits,* 1902; *The Americans,* 1904; *Principles of Art Education,* 1904; *The Eternal Life,* 1905; *Science and Idealism,* 1906; *On the Witness Stand,* 1908; *Aus Deutsch Amerika,* 1908; *Psychotherapy,* 1909; *The Eternal Values,* 1909; *Psychology and the Teacher,* 1909; *American Problems,* 1910; *Vocation and Learning,* 1912; *Psychology and Industrial Efficiency,* 1913; *American Patriotism,* 1913; *Grundzüge der Psychotechnik,* 1914; *Psychology and Social Sanity,* 1914; *Psychology, General and Applied,* 1916.

As the titles of this list indicate, Professor Münsterberg was interested in a wide range and variety of topics. His earlier interests centered in the problems of physiological psychology, where he did pioneer service in opening up such topics as the phenomena of volition to experimental analysis. Then came a period during which his attention turned to the philosophical aspects of his science, and to questions of general human interest. The later years of his life were largely devoted to an endeavor to apply psychology to the practical affairs of life. Beginning with the psychological problems which confront the criminologist and the administrator of justice he gradually extended his investigations to the general question of vocational selection and industrial efficiency. For several years he had been active in a movement which aimed to promote friendly relations between Germany and the United States; and it was in consequence of his activities in this direction and in the direction of the practical application of psychology that he was most widely known. For more than two decades Professor Münsterberg has been a prominent figure in American psychology, where his commanding personality, his originality, his skill in forceful presentation, and his fearless and untiring devotion made him a leader in arousing and stimulating interest in psychological problems.

J. W. Baird.

Clark University.